

The moment the reading of the resolution was concluded, it was at once tabled by the practically unanimous vote of the House. There was no discussion. The members of the House cared for none. There was no time for the officers to express themselves even had they desired to do so. The statement that the resolution was opposed by the officers, the trustees or their friends is an absolute falsehood, unless the entire membership of the House of Delegates is included under the designation "friends." The Association and its officers are fighting secrecy and deception and are not practicing it.

11. That at Boston "a strenuous effort was made to get rid of the Editor, and that so great has become the dissatisfaction with his administration that this endeavor might have been successful had the general body any voice in the management of the Association."

This statement is unqualifiedly false, as all who attended the Boston session well know. There was an opposition, it is true, and a bitter one, but it was limited to the horde of nostrum vendors who have been getting rich by humbugging and deceiving our profession, and to the owners or attaches of those medical journals nourished by the same brood, who allow themselves to be used as the mouthpieces and tools of proprietary interests. The Editor of *The Journal of the American Medical Association* knows full well that he is hated with a most intense hatred by these gentlemen and their allies, and that they would stop at nothing to secure his "dismissal," but he also knows, and knows thoroughly and well, that the "management" was fully and completely endorsed by 95 per cent. of the physicians at the Boston session, and, for that matter, is endorsed by a like proportion of the physicians of the country, who know the truth of what is going on.

So much for the "facts" regarding the American Medical Association as set forth by the editor of the *Record*. Just a word as to that gentleman himself. He graduated in the spring of 1877 and so has been a member of the medical profession for 29 years. During all this time, his interest in the American Medical Association and in medical organization was so great and his enthusiasm so uncontrollable that in March, 1906, just ten weeks before the Boston session, he became a member of the American Medical Association. Truly, not without reason has the zeal of the new convert become proverbial. Had he devoted a small part of the 29 years of his professional life to a more careful study of the organization of which he was, at last, to become a member, he might have been able to discuss the Association's affairs intelligently, if not truthfully. During all this time, men whose membership in the organization numbers years while his numbers weeks were earnestly and unselfishly striving, not only to build up the American Medical Association, but to bring about better conditions in our profession. During the same time, what has the editor of the *Medical Record* done in this respect? Yet he now arrogates to himself the right to criticise and, by

implication and by baseless insinuations to brand these same men as "boodlers" and "grafters."

Is it probable that the business interests of an old and hitherto reputable publishing house will be enhanced by the use of its journal as a medium for such malicious, unfounded and unwarranted attacks? We think not. However, we commend this thought to the consideration of William Wood & Co. of New York.

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THE 74TH ANNUAL MEETING OF THE  
BRITISH MEDICAL ASSOCIATION,  
HELD AT TORONTO, AUGUST 24TH,  
1906.

(Reported for the JOURNAL by Dr. Langley Porter.)

It has been a great pleasure to see American and British physicians fraternizing at this, the 74th Annual Meeting of the British Medical Association, which has been, in effect, a gathering of medical men from the English-speaking world. Certainly there were as many visitors from the United States as from Great Britain, and in the sections many papers were read by Americans, and there were but few discussions that did not bring some authority from the United States to his feet.

The section that attracted by far the most attention was the medical, with its international symposium on heart block;—Barr, Gibson, Broadbent, Mackenzie, Erlanger, and Aschoff, each had a paper, and all were listened to with the utmost attention.

Aschoff, Professor of Anatomy at Marbourg, created a profound impression by his exhaustive detailed paper and impressive array of specimens, illustrating the monumental and revolutionizing work done by himself and Tawara on the anatomy of the conducting fibres of the heart. He demonstrated, apparently to the satisfaction of all the authorities present, that the conducting apparatus of the heart is inherent in an array of fibres partially described half a century ago by Purinje, which fibres consist of a tissue intermediate between the muscle and nerve, very rich in sarcoplasm. According to Aschoff the system begins at the roots of the great veins, spreads sub-endocardially over the auricle, is gathered together at the auriculo-ventricular septum, into what Aschoff calls "the knot", which corresponds to the bundle of His, and from this again spreads out, dividing and re-dividing sub-endocardially over the ventricle, the final divisions merging with the true muscle plasm. The distinctive features of these fibres, according to Aschoff, are their richness in sarcoplasm, the length and narrowness of the individual cells, and the presence about them of a true sheath of connective tissue. It is the belief of the German anatomist that interference with any part of this conducting apparatus may cause arrhythmia of one or another type; perhaps complete heart block. He claims to have seen two cases of typical heart block, in which the His bundle was unimpaired, although he did not make it clear whether or not any part of his conducting system was injured in these cases. He showed slides and photographs from the case reported by Ophuls and

Schmoll of San Francisco, and quoted extensively from Schmoll's paper on heart block in the course of his talk. Aschoff also showed, as a unique specimen, the heart of a patient who had suffered from acute rheumatism, in which there was a nodule that had destroyed the continuity of the Purkinje fibres, and he claimed such a specimen could occur in no other disease, his idea being that the endocarditis of rheumatism was not accompanied, as usually thought, by myocarditis, but that the danger to the myocardium was secondary to interference by these specific nodules with the conducting apparatus, such nodules he conceives as being practically the same as those so often seen sub-cutaneously in cases of acute rheumatism.

There was some skepticism manifest, and the just criticism was made that there was more interpretation than demonstration in this particular case.

Mackenzie, of Burnley, was received with great applause, and spent a large part of the time allotted to him in complimenting Aschoff on achieving a piece of work that could explain so many hitherto inexplicable points in the production of arrhythmia and heart block.

Mackenzie gave a short but very clear exegesis on the progress of the Adams-Stokes syndrome, and showed tracings with clinical notes and explanations which made this complicated subject quite clear to his audience. He also demonstrated the newest developments of his famous polygraph,—an ink-writing machine that will trace the radial and venous pulse and give the tracings of any desired length. The apparatus is exceedingly simple, but, unfortunately, the instrumental defects are so great that the contour of the tracings has no value. However, it will give as perfect relative time results as possible, and it is obvious that we will be able to get many new facts from an apparatus that can produce for us time relations between auricle and ventricle through a period of from ten to fifteen minutes. Especially will it be useful in those cases of occasional arrhythmia that are so puzzling to us.

George Gibson, of Edinburg, followed Mackenzie, and showed that in heart block it is possible to record the independence of auricle and ventricle by use of the capillary electrometer, and also that such independence may be demonstrated by the Roentgen Rays on the fluorescent screen. Gibson had a series of beautiful microscopic sections of the His bundle, from normal hearts and from hearts of patients exhibiting heart block, and he was the one man of authority who seemed to think that Aschoff's interpretations of his findings were, perhaps, overenthusiastic.

Erlanger's paper was a re-statement of the facts published last year, embodying the physiological experiments that have now become classic and familiar throughout the United States. Erlanger had some friendly controversy with Mackenzie as to the source of the stimulus to the auricle, maintaining that it was probably entirely due to the entrance of the blood. I believe he has gone to Madison, with the determination to investigate this point more fully, and that very soon we may expect to

have more exact knowledge than we have hitherto had about the initiation of stimuli.

While the section of medicine was the one that attracted, perhaps, the largest audience, the greatest individual impression was made by Sir Victor Horsley, who delivered the address in surgery. Naturally, Horsley was given a subject in the domain of the nervous system, and the title of his paper was "On the Technique of Operations in the Central Nervous System." He occupied some hour and a half, during every minute of which he demonstrated his mastery of the subject. The mover of a vote of thanks very justly stated that, Horsley had not quoted from literature, because he had made the literature on this subject, and that the paper just presented was a contribution that epitomized all his past efforts and presented many new facts.

The points of most interest that were brought out related to the prevention of shock in brain operations. Horsley attributed the causes of shock, first, to anesthetic, second, to the cooling of the patient's body, third, to the cooling of the brain tissue, and fourth, to pressure on the bulb, due to faulty manipulation and position, and he was exceedingly insistent on the matter of anesthetic, going into the subject in great detail.

He stated that years ago, because of the remarkable power morphine had of contracting cerebral vessels, he used a combined anesthetic of morphine and chloroform. Because of adverse effects on the alimentary canal and on the respiratory centre, he gave up the use of morphine and has since employed pure chloroform. He had not used the intraspinal injection of cocaine or stovaine in operations. Reflecting on the reports, especially from Morton, of San Francisco, he had decided to give that method in the future a thorough trial. Ethel he condemned as inadmissible, although he specially stated that he did not desire to be considered as criticizing his colleagues, especially American surgeons, who had accomplished most brilliant results under ether narcosis. His objections to ether were that it causes a rise of blood pressure with a notable increase in blood vascularity, and, therefore, increased hemorrhage. These disadvantages he thought outweighed its lower physiological toxicity on nerve tissue.

Chloroform, on the other hand, had been shown to cause a fall of blood pressure with the vascularity of blood diminished, although not entirely absent. So, it does not aggravate bleeding, nor embarrass respiration, and is followed by no after-excitement and but moderate headache. To the mind of the lecturer it seemed probable that sickness after chloroform can be entirely done away with, as it probably depends on the dosage.

He referred at length to the report of the British quantitative determination of the amount of chloroform, which was given the task of making a precise quantitative determination of the amount of chloroform necessary to produce anesthesia. The committee reported that less than two percent of chloroform vapour in the atmosphere, breathed by the patient, was enough to produce deep narcosis, and that a very much smaller dose is required to maintain

unconsciousness. Since that report Horsley has used entirely the Vernon-Harcourt inhaler, and he showed a photograph of his amphitheatre with the apparatus in use. It seems rather cumbersome, but if his claims are justified, which they undoubtedly are, this clumsiness is more than compensated for by the increased safety.

The inhaler is arranged to give from one-half to two percent volume of chloroform. Many anesthetists have reported adversely on the instrument, Horsley believes, because it has been improperly used. He lays great stress on the absolute necessity of making the face piece fit tightly by the use of antiseptic towels. He also gave us a diagram, which diagram illustrated graphically the pain undergone by the patient during the operation. Skin incision gave the highest point in the curve, which dropped rapidly during the operation on bone, with a slight raise of the pain curve, due to the fifth cranial nerve supply, when the dura was opened, then it very rapidly fell to almost nil during the exploration and treatment of the brain. There was another sharp raise when the skin was sutured, followed by a rapid fall when the dressings were placed. His point was that in giving the chloroform, the curve of chloroform vapor concentration should run parallel with the curve of the pain production, and in the practice of his anesthetists, Doctors Buxton and Powell, it is customary to begin with one-half percent vapor, which is rapidly run up to two percent. This is maintained for about five minutes, before the incision of the skin and reflection of the flap is accomplished. The dose is then lowered and the bone removed at about one percent concentration. Before the dura is opened concentration is increased to somewhere about one and one-half percent. Afterwards the encephalon can be dealt with without causing any pain unless some of the peripheral sensory cranial nerves be accidentally irritated. Consequently less than a half percent of the chloroform in the air is needed here. Often it can be entirely shut off. Horsley reports that he has been able to do this for twenty minutes in a child, never more than fifteen minutes in an adult. Before the return of the reflexes, of course, renewed concentration of chloroform is administered. After the brain lesion is dealt with the percentage is raised about one percent during the insertion of sutures. Finally this percentage is continued to the commencement of the dressing in order to prevent vomiting occurring before the protective dressing is applied.

In the matter of maintenance of body temperature, Horsley insists that the temperature of the room should not be less than 75° F., and that the operating table should be provided with suitable heating appliances, either of hot water or electrothermic.

In order to maintain the physiological energy of the nervous system and to prevent radiation from the brain, he uses continuous irrigation with a solution of sublimate of 1 in 10,000 strength, which solutions he puts into the irrigator at a temperature of 115°.

On the question of hemorrhage, which is very annoying during operation, he urged the advantage

of using irrigations. He quotes Milne Murray, who from his experiments concluded that water from 70° to 105° F. will invariably dilate blood vessels and promote the flow from open ones, but that water of a temperature from 110° to 120° will have just the opposite effect. Therefore, his irrigation at 115° answers a double purpose, preventing hemorrhage as well as radiation. He says it is very essential that a large irrigator should be used, in order that the fluid should remain at 115° and not fall below 105°.

He very strongly objected to the tying of the carotid in order to control the blood supply, and thought it did great damage. All bleeding from the veins and sinuses in bones, he held, was trivial, because it could be immediately arrested by plugging with wax if the periosteum around the hole were completely removed; no difficulty should ever arise from hemorrhage from this cause. Bleeding from sinuses and other veins and from large vessels in general can be controlled by pressure with the point of an instrument, while the opening is closed by a fine suture.

He also alluded to the inhalation of oxygen as a manoeuvre by which venous oozing could be rapidly controlled, and one of the advantages of the Harcourt inhaler is that at any time during the proceeding, without any special preparation, oxygen may be passed into the inhaler.

His remarks on shock contained little except what is common knowledge.

As to the methods of operation Sir Victor Horsley thought that much of the shock depended on the manner in which the skull was opened. The principles that he laid down were, first, that the bone should be divided with as little vertically applied force as possible and removed with the least possible pressure on the brain and dura. He finds that these essentials may best be fulfilled by first removing a trephine disc, and marking with a large saw the area to be removed, and finally cutting away the bone with a large bone forceps, all traction being directed outwards. He was particularly scornful of those operators who use mallet and chisel.

He also laid down as an axiom that if a line be drawn from the frontal eminence to the occipital protuberance, shock results more often from operations below than above that line. He also stated that the risk of an operation for decompression is greater if the opening is not made directly over the lesion. He laid stress on the point that operation gave a greater risk when the diagnosis had not been made, or had been incorrectly made. In cases incorrectly diagnosed, 37% of patients had died from shock, and in those cases in which the diagnosis had been correct, only 7%.

He was very urgent in his recommendation of the injection of strychnine combined with oxygen inhalations in the treatment of the respiratory phases of shock. He thinks as a stimulant to the bulbo-spinal centres, strychnine is unrivalled, but it seemed to him inadvisable to give the drug before some alteration in the rhythm of the respiratory centres showed itself, and very unscientific to give it either immediately previous or at the end of an operation,

with the idea of anticipating shock. He also called attention to the fact that we are often led into fear of shock by the normal depression of the cardiac and respiratory functions that precedes chloroform vomiting. He admits, as far as circulation is concerned, the benefit of bandaging the limbs over cotton-wool, and of repeated nutritive enemata. In very acute conditions a small dose of atropine is useful, and in cases of peripheral vasomotor paralysis a small dose of digitalis. Alcohol he considers as useless—in fact, harmful; strong coffee, useful; but cardiac stimulation generally a clinical error under the circumstances.

The lecturer spent some little time in discussing the displacement of the brain in the operation. He was thoroughly convinced that Frazier's recommendation to remove the lateral hemispheres of the cerebellum is not only useless but definitely damaging. He thinks that the manner of displacing the brain to reach the tumor is a matter of great importance. He advises pressure directly upwards on the vault, but this compression must be gradual and cautious and produced by the insertion of a flat spatula beneath the hemispheres. Under these circumstances the soft nerve tissues mould themselves readily, but too much and too rapid pressure will produce laceration of brain tissue and oozing between the fibres of the corona. Such compressions at the base are relatively unimportant. With this procedure properly applied to the temporal lobe, one can see and readily examine, with good illumination, the crura cerebri, the circle of Willis, the pituitary body and internal carotid, even the second and third nerves come into view, and he has, after the removal of a pituitary tumor, been able, by the use of a small rhinoscopic mirror to further examine the base of the brain. With a copper spatula of suitable size, gentle pressure and a strong headlight, the lateral region of the cerebellum and medulla oblongata with the issuing nerves, may be brought into view, and as he has demonstrated these facts, the lecturer is especially chary of following the procedure of removal advised by Frazier.

Reference was made to the superstition that interference with the ventricular cavity is necessarily fatal.

One important conclusion of the lecturer was that in no case in which there had been signs of intracranial pressure, especially if there was increasing optic atrophy, had the medical man a right to temporize. Six to eight weeks of medical treatment, which practically means treatment with iodides only, was the limit to be allowed. Incision into the dura, no matter if the tumor was inoperable, would in most instances mean the retention of sight by the patient for the period of remaining life, and in many more cases than medical men in general believe, the tumor would be removable and the condition amenable to surgical interference.

Dr. Adami's paper on the relation of fluid crystals to arterio-sclerosis and other pathological conditions is one that will probably create a great deal of attention when published.

So much was programmed that the attempt to follow the work was something like following the per-

formance in a three ringed circus. For instance, while heart block was being discussed in the section of medicine, a very interesting discussion, in which Professor Clifford Allbutt was the most notable speaker, was being carried on by the section of pathology, which was largely given over to a discussion of Professor Hewlett's and Dr. De Korte's papers on a beri-beri-like disease in monkeys, and gave a very full pathological discussion of the subject. Dr. Hamilton Wright was also present, and gave his well-known views.

The joint discussion by the sections of medicine and pathology on nutrition brought a statement from Dr. Chittenden of his experimental work on the nitrogen minimum. Professor Haliburton was inclined to doubt the utility of the work of Chittenden, laying great stress on the fact that nitrogen minimum was not necessarily the nitrogen optimum, and he further laid stress on the fact that while Chittenden's experiments had been extensive, that more extensive experiments were being performed daily on the poor in great cities of the world and on vegetarian nations who lived on a nitrogen standard, such as Dr. Chittenden suggested for us, and they were inevitably people below the standard in physical power. Robert Hutchinson gave an exceedingly interesting philosophical discussion of the whole subject, also insisting that the nitrogen optimum was not necessarily the nitrogen minimum. He asked us to consider that man could and did get along with the use of one lung, but that any one would probably claim that one lung was not physically optimum. His suggestion was it would be exceedingly interesting to follow carefully in the future the incidents of disease and their resistance by the people, who accepted Chittenden's standard. He criticised Dr. Chittenden's method of demonstrating blood resistance, Chittenden having laid stress on the hemoglobin contained; whereas to the mind of Hutchinson the only method we had which gave us any idea of the resisting power of the body fluids to disease was the determination of the opsonic index.

As a matter of fact, in all the learned discussions practically nothing new was brought up, and Chittenden's position remains unshaken. Hutchinson made one good point, that we should be much more intent on determining, not the nitrogen or the carbohydrate, or the fat minimum or maximum or optimum, but the food optimum; that undoubtedly the proteid values of both fats and carbohydrate were insufficiently recognized and insufficiently used in the formation of normal and sick dieteries.

The writer was, naturally, most interested in the section of pediatrics. Unfortunately the interest caused by the presence of Aschoff, Erlanger and Mackenzie in the medical section prevented the attendance at all the meetings of this section. There were, however, very excellent papers by Morse, of Boston, and La Fetra, of New York, on the treatment of summer diarrheas of children, which in the minds of the speakers, seemed to be largely matters of diet. Shaw, of Montreal, gave a very exhaustive and interesting account of the fat digestion and absorption in children, bring out no particularly new

facts, but reviewing old facts in a way that will certainly be of value to those of us who have to feed children whose stomachs seem unable to digest normal percentages and usual mixtures of fat. The most interesting discussion in this section followed the papers of Cautley and Stiles, who discussed the condition of congenital pyloric stenosis. Specimens were exhibited which could leave no doubt in the mind of any one that the condition is truly congenital, and not, as some claim, post natal and due to attempts of a dilated stomach to empty itself. Ashby showed us a specimen from a child thirty-five days old, in which the pylorus was hypertrophied to at least half an inch in radius. The stomach itself was dilated and would hold probably about eight ounces. Other specimens were shown illustrating the same point.

Cautley dwelt upon the medical treatment and on diagnosis. His conclusion was that if one waited for the production of a dilated stomach which should show peristaltic waves and a tumour sufficiently large to be easily palpated, the general condition of the child would be such that operation would give it no aid. Stress was laid upon the scantiness of the stools, although this was by no means a necessary clinical point, for the case which Ashby showed allowed enough food to enter the intestine to provide a fairly bulky stool, but still that point, together with continuous vomiting must be one that leads up to operative interference. Such conditions are exceedingly rare. In twelve years he had seen fourteen cases, and he undoubtedly has an opportunity few men have to see cases of such a type. The great difficulties in diagnosis may be well imagined. For diagnosis Cautley divided the cases into three classes: (1) True productive congenital hypertrophy; (2) Slight congenital hypertrophy with post natal spasm and dilatation; (3) Pseudo hypertrophy due to spasm. Hutchinson created somewhat of a sensation by stating that in the last two years he had seen ten such cases, of which none had died, and he had initiated surgical interference in none of them, which results were quite opposed to general experience. Cautley went so far as to state that if Hutchinson had seen ten cases in two years, and they had recovered without operation, none of them were true congenital stenosis of the pylorus, that they were merely cases of spasm secondary to acute indigestion, Cautley's third class. In the discussion one fact was not brought out that seems to me of great importance, that is, the readiness with which certain fats, high in caprillic and caproic acids, can produce and do produce, spasm and a condition of pseudo stenosis, with dilation and vomiting, which is very difficult to diagnose, but which responds readily to the use of alkalies, lavage and minute doses of opium.

Stile's paper treated entirely of the surgical aspect, and his results were certainly not such as would encourage us to proceed to operation unless our case was one in which there was absolutely no question of true congenital hypertrophy, and in which the diagnosis was made fairly early. Forty-eight per cent of his operative patients died, and he probably has the average experience. He divided

the procedure into divulsion and gastroenterostomy and pyloroplasty. He advised the latter operation, the v-y operation, as he called it, appealed more to surgeons than it does to me. He advised against the use of the gastroenterostomy, and divulsion had produced no good results in his hands. He strongly opposed the dilating of the pylorus as being thoroughly unscientific and dangerous, stating that unless the pylorus was ruptured no good could come from it and it was much better to cut than to rupture, avoiding all danger of infection in the peritoneum.

The section of therapeutics, under the Presidency of Donald MacAlister, Cambridge, was in joint session with the section of medicine for one day. Most of the programme was given up to the value of alcohol in treatment, the opinions being diametrically opposed, but the final conclusion seemed to be that while alcohol was of use, especially in those case in which the principal vessels were contracted, it was of little value in any other way, but great stress was laid upon the difference between alcohol pure, dilute, and alcohol in combination with the higher ethers, as in the better class of wines, most of the speakers laying stress on the fact that wines, such as sherry, and champagne, were of great value, when diluted alcohol was practically useless.

The meeting closed on Friday, and on Saturday the members were dispersed throughout the country, for a series of excursions and fetes.

### SPASMODIC TORTICOLLIS.\*

By P. C. H. PAHL, B. S., M. D., Los Angeles.

Synonyms—Caput obstipum spasmodicum; torticollis fonctionnel; tic rotatoire; torticollis mental; spasmodic wry-neck; torticollis spasmodicus; tic giratoire.

Introduction. About three years ago, I was called in consultation upon a case of spasmodic torticollis. It was the first case that ever came under my observation, and my having examined, up to that time, not less than two thousand orthopedic cases of all kinds, goes to show that the condition is a comparatively rare one.

I began looking up the subject in the literature at my disposal, and I was very much disappointed at the meagre information to be obtained.

I went ahead with my case, however, and did the best I could, determining that I would look up this matter exhaustively and bring all the facts obtainable together into one paper, which I would present to the Western profession, hoping that it might serve as a help in recognizing the condition and in choosing the method of treatment most applicable.

This article contains the report of cases and other information of value to be found in the papers of fifteen American, ten English, nine French, five German and two Italian authors. The different methods of treatment and the results of these are given in full, as well as three of my own cases and

\*To have been read at the Thirty-sixth annual meeting of the State Society, San Francisco, April, 1906.